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THE PLAGUE NOT CONTAGIOUS

OR

A DISSERTATION

ON THE SOURCE OF

EPIDEMIC AND PESTILENTIAL DISEASES;

IN WHICH IS ATTEMPTED TO PROVE, BY A NUMEROUS
INDUCTION OF FACTS, THAT THEY NEVER ARISE FROM
CONTAGION, BUT ARE ALWAYS PRODUCED BY CERTAIN
STATES, OR CERTAIN VICISSITUDES OF THE
ATMOSPHERE, etc.

THE SECOND EDITION, WITH ADDITIONS.

BY CHARLES MACLEAN, M. D.
MEMBER OF THE CORPORATION OF SURGEONS OF LONDON;
AND, FOR SEVERAL YEARS, A PRACTITIONER OF
MEDICINE, IN THE EAST INDIES.

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INTRODUCTION.

In January 1797, this dissertation was first published, in Bengal: it has since undergone several republications, in other countries. But the arguments it contains, against the existence of contagion, in epidemic and pestilential diseases, if a judgment may be formed from public documents, do not seem to have yet produced general conviction.

By the multitude, as well as by Governments, and even by Physicians, epidemic and pestilential diseases continue still to be regarded as contagious: and ships, arriving, from New York, from Philadelphia, and from Egypt, are obliged to perform quarantine, as formerly, in England, in France, and in other enlightened nations.

It might have been expected, that the people of America, at least, profiting by repeated and woeful experience, would have shewn a greater readiness to investigate, and to adopt, salutary truths, respecting a calamity of so frequent occurrence, and such fatal effects, among them. But, as far as I have been able to learn, although the arguments in this dissertation have met with a reception not altogether unfavourable, in America, the conviction that yellow fever, and other epidemic diseases, do not depend upon contagion, is yet exceedingly limited, in that country (*).

(*) See the recent American reviews, and medical publications. The New York Repository, for 1798, contains the most candid criticism I have yet seen, of theories and experiments, published, several years ago, in Bengal, of which this dissertation formed a part. — Some criticisms, or rather condemnations, which have lately appeared in England, of the essays in question, are so remarkable for their indecency, total want of candor, or total ignorance, that, at present, I am of opinion I ough not to condescend to enter into a refutation of them. I had once, indeed, some notion of prefixing them, as recommendations, to a new edition of my medical tracts. But as even tha would be to confer on them an unmerited importance, I hav thought it better to refer such of my readers, as may choos to take the trouble of perusing them, to the London Monthl review for April 1799, and to the London Monthly Magazin for July 1799, where they will find the same observation repeated, in nearly the same language. — Admirable coincidence of critic brains, or sympathy of critic stomachs!

Upon this subject, I have now very little, that is new, to add. All the reflections, furnished by sub-

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All the reply I shall here make to the very *modest* remarks contained in these publications, is, that it would give me infinite pleasure to have an opportunity of opposing my medical skill, to that of any reviewer, from the highest attic, to the lowest cellar, in *Grub Street* (*), our patients, or the whole world, being judges between us. As, however, the anathemas in question may possibly not be of Grub Street origin, I would extend this observation generally to all the indiscriminating opposers of the doctrine of excitation, in every part of the globe. The unwarrantable, undecorous, and unmanly manner, in which my medical reputation, and, through it, my moral character, have been assailed, by those periodical retailers of fame, the English monthly reviewers, will, I trust, justify me to the PUBLIC, for the seeming arrogance of such a defiance;—the only kind of replication, which, at present, their comments appear to me to deserve.—But should it, at any time, be expected by

(*) For the information of foreign readers, who may be unacquainted with the name, it will be proper to mention that *Grub Street* is a place in London, noted for being the chief residence of critics, minor poets, and other writers of inferior rank, who are said, for the most part, to be in the pay of the London Booksellers, and are known too often to get their living by corrupting literature into the basest of trades; — thus leaving it somewhat doubtful whether, upon the whole, they are most to be condemned or pitied. At no preceeding aera, in the history of the country, has venality, in this line, been carried farther than it is, at present, carried, in England. To describe the extent, to trace the causes, and to point out the consequences of so enormous an evil, would be entirely foreign to my purpose here. But it is an enquiry highly deserving of investigation.

sequent experience, have tended, not only to confirm the conclusions, I before drew; but even to render belief in the existence of contagion, in any of thos general diseases, which affect men more than once during the progress of life, altogether as absurd, an inconceivable to my mind, as a belief in the realit of any of the tales of the Genii.—

A fact being once thoroughly taken for granted the most palpable contradictions will be admitted an reconciled, in order to account for it.—When, for instance, a ship arrives in England, from the Mediterranean, it is implicitly believed that the infection o the plague can be communicated to others, by the crew of that ship, although they themselves have not nor, in the whole course of their voyage, have had a single symptom of the disease. This inference which, to any mind, capable of strict reasoning

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them (the public), or should it promise any advantage t the cause of medical truth, however despicable the arguments of my opponents, I shall, by no means, declin entering, at greater length, into the merits of the question.

They may be assured that no other motive than contemp restrains me: for, although I freely return their pity, cannot think of repaying their indignation.

must appear utterly impossible, is, however, an impossibility, of which it will, I fear, be very difficult to eradicate the general belief.

For my own part, having endeavoured, as far as very limited means and opportunities could go, to elucidate this important subject, I must leave it to others, who may be more fortunate in these respects, to prosecute the investigation farther.—In disproving, theoretically, the existence of contagion, in several diseases, in which it had, always, implicitly and universally been taken for granted, I think my endeavours have been attended with a success, which will ultimately prove decisive. But it has not fallen equally within my power to collect, in these diseases, that experimental detail, which, although often fallacious, and misapplied, is however the most certain means of producing conviction, in minds of the ordinary scope of thinking.

Struck with the doctrines advanced in this dissertation, or having themselves, perhaps, entertained similar ideas, some physicians have lately obtained missions, from England to Constantinople, with the intention of attempting to ascertain, by experiment, how far the plague is actually contagious, and wha is the best method of curing that disease. From th intreped spirit of investigation, with which I kno one of these gentlemen to be animated, much usef information may, I think, be augured to the publi But the field is wide, and will admit, with advan tage, the co-operation of many labourers. Th French physicians in Egypt will, no doubt, hav availed themselves of the opportunities, their situ tion, for some time past, has afforded, to impro this branch of medical knowledge.—The accumulati experience of America, during the late frequent o currence of yellow fever, in some of the cities of th continent, should add much to the stock of gene information. And the practioners of Medicine, in lar cities, on the continent of Europe, subject occasiona to the ravages of epidemic diseases, will, no dou contribute their share, when unfortunately opportu ties occur, towards the establishment of certain a fixed rules, for the future treatment of those wid destructive maladies.—From such great evils, should endeavour to extract some good.

But, as the disunited efforts of individual tale without the support and protection of the constitu chief authorities of states, can give but slow progression to improvements, it is most devoutly to be wished that, for the more speedy completion of the important object, here offered to the attention of the public, some establishment or establishments, powerfully protected, and liberally endowed, may be forthwith instituted. It would, then, be no longer one of the chief reproaches of the age, that governments, and societies of learned men, suffering themselves to be amused with dissertations, rather ostentatious than useful, on the antique curiosities of distant countries, neglect to encourage investigations far more essential to the general happiness of their species. The sources, symptoms, and cure of such destructive epidemic diseases, as have obtained the name PLAGUE, are phoenomena not less curious, and certainly not less interesting, than those, which relate to inanimate matter!—Successfully to point out the means of guarding against, of curing, or even of alleviating those terrible pestilences, which yet continue so frequently to desolate the most populous cities of the old and new world, would be, in my opinion, to merit the approbation of every community.—Is it a visionary hope, that the question, respecting the existence of contagion, in plague and other epidemic diseases, may, in no long time, become a more fashionable topic of discussion, with travellers, than even the pyramids of Egypt, or the source of the Nile?—

Considering the applicative importance, no less than the speculative novelty, of the doctrines here advanced, I conceive it, in some measure, incumbent on me to state;—that, should any government, corporation, or individual, possessing the power, have also the inclination, to institute a practical enquiry into the nature of the plague, and of other epidemic diseases; and should it be thought that, from having paid more than ordinary attention to this subject, and placed it in a new point of view, I might be able to contribute, in any manner, to the successful prosecution of such an enquiry, I shall, at all times, be ready, if called upon for that purpose, most chearfully to devote my time and labour, to confirm, by actual experiment, the theories contained in this dissertation.

The determination of the question, respecting the contagious nature of pestilential diseases, is in itself of such great and general importance, and the interest, which I feel in it, naturally arising from conviction, is so strong, that, were the means in my own power, I should not hesitate a moment to undertake the investigation. At all events, the observations I have made may excite enquiries, which might not otherwise have been so soon thought of: and this reflection will be an adequate, if my only reward, for having suffered the obloquy of professional innovation.

The mode of treatment, which, from the strictest medical analogy, I venture to promise will be found successful, in the cure of plague, and, in general, of all epidemic and pestilential diseases, is deducible from the principles, explained in my essay, entitled „A view of the science of life etc." and in my „treatise on the effects of mercury upon living bodies, and its application for the cure of diseases".

Whether, in treating the plague, mercury, or other stimulant powers, be used, success, I take upon me with confidence to predict, will depend upon the application of them, in a very high degree, and a cautions and gradual subduction of them, in the manner, and upon the principles explained, in the publications above mentioned.

There are two observations more, to which, before we part, I would earnestly solicit the attention of the philosophic reader, for whom alone I write. The first is: that, in the application of exciting powers to living bodies, no danger can arise from their degree of intensity, whilst it is such as not immediately to destroy organisation: the whole danger arising from their undue subduction. If, therefore, any of what I have termed the extraordinary stimulant powers could be applied to living bodies, with as much constancy, and equality of force, as the ordinary ones of air, oxygene, caloric etc., no danger would arise from their uniform action, whilst it remained within the above mentioned boundary.—The second remark as it will account for a very great number of medica mistakes, ought, in my opinion, upon every prope occasion, to be repeated: namely—that, when th effects of the powers, directed to be applied, in an given case, do not correspond with the usual and ordinary results, in similar cases, the veracity of th patient, the judgment of the Physician, or the qualit of the medicines prescribed, are *always* to b suspected: for the laws of nature NEVER err.

Some attempts, I understand, have lately bee made, to ascertain whether the plague can be communicated by innoculation. How far these attempt may have succeeded, as I have not had an opportunity of perusing the tracts published upon the subject I do not at present know. But, at all events, i appears to me that the practical consequences, to b deduced from these experiments, cannot be o much importance. For, if it even were true tha fever and all the other symptoms of general debility which characterise plague, could be produced b innoculation, and if this disease, as is at present almost universally believed, can be communicated t the same person, repeatedly during the progress o life; that practice would scarce ever be resorted t by persons in health, who had any chance of escapin the disease. All the inference, therefore, that coul be deduced from such a fact, supposing it to be true is that the plague is, to a certain degree, contagious.— But, as so many apparently irrefutable argument prove the contrary, it seems much more probable tha local affection only can be produced, by innoculating ith the matter peculiar to plague; in the same man- er that pustules arise, from applying the matter of nall-pox to the skin of persons, who have already ad that disease, without, however, producing the ightest degree of general affection.—

If, then, which is most probable, the matter ius inserted produce only a local affection, such as iay arise from the application of any irritating matter hatsoever to the skin, the disease, as a general one, nnot justly be considered as contagious.—Some triking instances might be adduced, to shew that ven general diseases, which arise from local contagious fections, are not themselves contagious. But to ter, at greater length, into this analogy, would be ere misplaced. All that has been said, indeed, specting innoculation for the plague, as the facts dmit of such easy probation, may, perhaps, be eemed superfluous.

Whatever may be the result of these experiments ltimately, in determining how far plague is a conta- ious disease, it can in no degree affect the reasoning, ther in this dissertation, or in my other essays al- ady quoted, respecting the method of cure, to be ursued in epidemic diseases in general, or even in ague itself.

Hamburgh, May 1800.

DISSERTATION etc.

In endeavouring to promote knowledge, it may sometimes be as useful to correct ancient errors, as to promulgate new discoveries. In medicine, doctrines of the greatest importance have been handed down, from generation to generation, which, although demonstrably false, have never once been called in question. The supposed existence of contagion, in plague, dysentery, and fevers, appears to me, to be a very remarkable instance of this propensity in man, to pursue the beaten tract, however unprofitable or unsafe. Is not this conduct often the effect of selfishness, choosing to avoid the responsibility of innovation? And is it not for the same reason, that erroneous doctrines generally remain longer undisproved, in proportion to the extent of their influence upon practice? However that may be, it is certain that contagious matter has, in all ages, been considered, by the multitude, as the cause of plague, dysentery, and fevers, — by far the most destructive diseases that affect the human race. And this hypothesis, upon the belief of which must have depended, and may yet depend, the lives of so many millions of our fellow creatures, seems to have been implicitly assented to, by every physician, from Hippocrates, to the present day.

Although I had long entertained doubts upon this subject, it was not till very lately, that I was led to consider it, with particular attention. Upon perusing Dr. Rush's publication, on the yellow fever, which desolated Philadelphia, in 1793, all my former doubts recurred, with tenfold force; and the history of that epidemic, served to complete my conviction, *that no general disease, which affects the same person more than once during life, can ever be communicated by contagion.* But as this term may be variously understood, it may not be improper to give a definition of it in this place. Contagion, I conceive to be—a specific matter, generated in a person affected with disease, and capable of communicating that particular disease, with or without contact, to another.

Was it a matter of mere idle speculation to inquire into the truth of this hypothesis, not less respectable from its antiquity, than from the universality of its adoption, I should have been entirely silent. But the frequent occurrence, and great mortality, of epidemic and pestilential diseases, in many parts of the globe, render it an object of the most essential importance to ascertain, whether they are ever contagious. That they never arise from that source, I shall endeavour to prove:—

1stly.—By shewing that consequences would necessarily result from the existence of contagion, in epidemic and pestilential diseases, which do not actually take place.

2dly.—By shewing that the existence of contagion, in these diseases, has always been taken for granted, not only without proof, but even contrary to the evidence of numerous and convincing facts.

3dly.—By pointing out the real source of such epidemic and pestilential diseases, as have usually been reputed contagious; viz. a certain state, or certain vicissitudes, of the atmosphere, together with the casual application of other powers, producing indirect debility*.

I.—Consequences would result from the existence of contagion, in epidemic and pestilential diseases, which do not actually take place.

If a person be affected with any contagious disease, it will necessarily be communicated to every other person, who comes within the infectious distance **, and is not at the time labouring under some disease higher in degree †. But it is well known, to every one conversant in the subject, that, in plague, dysentery, and fevers, a very small proportion only of those who come within what may be supposed to b the infectious distance, or even in contact wit the sick, is seized with these diseases. In th most universal epidemic, it does not appear that a tenth part of the whole inhabitants of a city, has ever been, at one time, affected. But, let it be supposed, that every sixth person might have been seized; is it credible that the remaining five sixths were, either not within the infectious distance, or were, at the time, labouring under some disease higher in degree, than the prevailing epidemic? If it even be admitted that, in a terrible pestilence, one half of the inhabitants of a city may possibly be affected, the supposition that the other half could escape, if the disease was contagious, would be still more extravagant; for the greater the number affected, the less chance must any individual have, of being exempted from contagion. Allowing that one person in ten may not have been within the infectious distance; and that one in a hundred may have been labouring, at the time, under a more severe disease, than the prevailing epidemic; such is the exact proportion that would escape. The reverse, however, is probably true. But whatever may be the proportion of the number seized, to those that escape, it cannot be doubted that the application of the powers, which produce the disease, in the person first affected, is adequate to produce the same effect, in all those, who are subsequently seized.

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* When any reference is made to general medical principles, in this Dissertation, they are understood to be thos of the Elementa Medicinae Brunonis, with certain modifications of that doctrine, contained in a book entitled: "A Vie of the Science of Life, etc."

** By infectious distance, I do not mean to express an definite space, but merely the distance, whatever that ma be, at which contagion is supposed to act.

† Vide propos. VIII. View of the Science of Life etc.

As the fact cannot be denied, that a grea majority have escaped, after contact with persons, ill of diseases supposed to be contagious attempts may perhaps be made to account fo it, by supposing a certain peculiarity of constitution, which exempts from, or disposes t disease. Is it the many, who escape, that hav this happy peculiarity of constitution; or th few, who are seized, that are so unfortunate a to possess it? The former are evidently to numerous to admit of such an hypothesis. Th property must, therefore, I conclude, be given to the latter. But a child, here and there, i exempted from small pox, although exposed t its contagion. In order to preserve a consistency, this fact must be accounted for, by th same, or another peculiarity of constitution Peculiarities of constitution, then, exempt from contagion, in one case, and dispose to it, in another: and thus a term, which in realit means nothing, may be made to account fo any thing. For my own part, I must confes my inability to comprehend any other *peculia* *rities of constitution*, or *idiosyncrasies of habit*, than what are constitutioned, by the different degrees of health and disease,—the different states of the excitability.

It appears, therefore, wholly unnecessary, for any purpose that I know, to suppose that, in epidemic and pestilential diseases, contagious matter is generated in those individuals who are first seized, and from them communicated to others; unless indeed, it be determined, at all events, to take the existence of such a power for granted.

It is a well known law of nature, that small pox, meazles, and other general diseases, which are unquestionably contagious, occur, in the same person, only once during life. It is also acknowledged by every author, who has written upon the subject, that plague, dysentery, and fevers affect the same person, as often as the powers, which produce them, are applied. Dr. Alexander Russel, affirms of the plague ‘the having had this distemper once, does not ‘prevent the contracting it again. I have seen ‘instances of the same person being infected ‘three several times, in the same season.” A similar observation is made by Dr. Rush, respecting the yellow fever of Philadelphia. "Cases of reinfection," says he, "were very common, during the prevalence of this fever."

Small-pox, meazles, and other general diseases, which occur only once during life, neve disappear, until the whole of those, who hav been within the infectious distance, and wer not, at the time, labouring under some diseas higher in degree, have received the infectio As these diseases are very mild*, children sometimes resist the power of contagion, fro the superior force of other diseases, althoug they may be so slight as to escape common observation. I will venture to assert that no person, in perfect health, ever was, or can be exposed to the power of contagion, without receiving the specific disease, which that contagion produces; excepting in small pox, meazles, etc. when the person has previously ha the disease.

Plague, dysentery, and fevers, then, as the are not subject to the same law, would, if the were contagious, never disappear. The contagion, meeting with no obstacle from other dis eases, severer in degree (for there are few indeed of that description) would exercise an unlimited and fatal sway. No person could escape. Those, who once recovered, would, again and again, be seized. Infection would proceed, in a continued circle, until the whole human race was extinguished.

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* They are not diseases of excessive excitement, but of indirect debility, and generally of a low degree.

2. The existence of contagion, in plague, dysentery, and fevers, has uniformly been taken for granted, not only without proof, but even contrary to the evidence of numerous and convincing facts.

It is well known that, in hospitals, camps, and ships, a very small proportion only of those, who sleep within a short distance of, and are frequently in conversation, or even in contract with, persons ill of typhus, dysentery, or fever, is seized with these diseases. So far from infection being invariably communicated in this manner, no instance of it has ever been distinctly traced. If such cases had even been recorded, we must either reject them as false, or abandon one of the fundamental axioms of philosophy. For, whatever has happened once, must happen often; it must happen always, in similar circumstances. But in the situations alluded to, these circumstances constantly occur, and the alledged effects do not follow. It is not fair to conclude, that dysentery is contagious, because one person happens to be taken ill, whilst in the neighbourhood of another, who has got the disease. If the conclusion were just, all within the infectious distance, not labouring under a disease higher in degree, would be similarly affected. They would have the disease with as much similarity of appearance, and equality of force, as children have the small-pox. In proportion to the number affected, the force of the contagion would increase. It would proceed in a geometrical ratio, diverging, from the center, to every point of the circumference, of a city, a camp, an hospital, or a ship. It is evident then, that in these situations, a contagion, which had the power of producing its peculiar disease, in the same person, more than once during life, would never disappear. But dysentery, fevers, and the plague itself cease, in all those situations, without having affected perhaps a tenth part of the community. They cease too, when they are epidemic, according to some periodical law, which evinces that they do not arise from any casual and uncertain source, like the accidental application of contagious matter.

The absurdity of the conclusions, which result from admitting contagious matter to be the cause of epidemic and pestilential diseases, could not have been overlooked till now, if the existence of such a source had not been so implicitly taken for granted, that even to call in question the truth of it, must, to many, have the appearance of excessive scepticism. But I shall endeavour to deduce my conclusions, from such numerous and undoubted facts, as ought, perhaps, to exempt me from that imputation.

During the prevalence of epidemic and pestilential diseases, it is well known, that nurses, and other attendants upon the sick, are not more liable to be affected than other people, who undergo an equal degree of fatigue. It may perhaps, be said, that they become habituated to the contagion. But how do they escape the first application of it? They have not then got the habit. No person of that description caught the infection from those who died, of what was called the jail fever, at the black assizes at Oxford; a case of alledged contagion so generally known, and so frequently quoted by authors. That the power, which occasioned disease at the Oxford assizes, was not contagious matter, is proved by its producing diarrhoea in some, whilst it produced fever in others. And further, no person was seized, who had not been directly exposed to the influence of the noxious air. Specific contagion, I conceive, cannot produce a disease, less uniform in its appearance, than small-pox and meazles. But every epidemic and pestilential disease, which has hitherto been reputed contagious, assumes such various and dissimilar appearances in different persons, that they cannot be the effect of any power, equal and uniform in its operation. The symptoms are not, in any two persons, exactly alike. Hence the difference of opinion among the physicans of Philadelphia, during their late epidemic; some asserting that every disease had resolved itself into yellow fever, whilst others, certainly with more reason, affirmed that the diseases of the city were various. No epidemic can become so general, as to suppress all other diseases; because all men, labouring under diseases of lower degree, are not exposed to the powers which produce an epidemic. The same person ndeed cannot, at the same time, have both a lysentery and a dropsy *; but every usual vaiety of disease may exist in a community, even n the time of a powerful epidemic, altho' the pidemic be the most general disorder. The issimilarity of symptoms, which occasioned his difference of opinion at Philadelphia, is, o me, a convincing proof, were there not nany others, that the yellow fever of that city, id not arise from any power, of such uniform peration, as contagious matter. Like wine, pium, or mercury, specific contagion must roduce similar effects, upon all men, who are imilarly situated. It must act alike, in Egypt, nd in America, in London, and in Constantiople. But, according to all accounts, the ymptoms of epidemic diseases, in different arts of the world, are very dissimilar; whilst hose of diseases that are undoubtedly contaious, such as small-pox, meazles, lues veneea, etc. are the same in all. Wine will in-

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* It is only meant, in as far as they are general diseases; r the local affections, which have obtained these names, they occur in different parts of the body, may readily o-exist.

toxiate, cathartics will purge, mercury will salivate, in all countries. They will produce thes effects, upon almost all men; certainly upo all men who are in health. Those only, wh are in a state of disease, higher in degree tha these powers can produce, will resist their operation *. But this proportion cannot be on in a thousand, perhaps not one in ten thousand Such also may be the proportion that woul escape, from the effects of a specific contagion applied to them. It is common however, fo men in health, to be exposed to contact with th sick, and to escape. In that case, contagion if the disease had been contagious, must inevitably have been applied; and without producing its imputed effects.

Was not the typhus fever, by which so man of the unfortunate people, who were imprisone in the black hole of Calcutta, perished, attende with an endless variety of symptoms? It doe not appear that the disease was, in that cas

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* It is impossible, with the greatest quantity of mercu that has ever been given, to salivate a person, whose liv is in a state of suppuration. No quantity of wine will i toxiate a person, ill of typhus fever, without having fi cured the disease.

ommunicated to any person, who had not
reathed the polluted air of the dungeon. Will
be said, that the Nabob Surajeddoullah had
reviously ordered contagious matter to be in-
rted into the black hole? If not, whence
as it imported, or where generated?

In the history of these diseases, I think it
ay be remarked, that physicians have been
culiarly exempted from their influence. Is
that there is a principle of repulsion between
edical skill and contagion? or is it not rather
r this plain reason, that these diseases depend
pon some other power, which the physician
better able to avoid? For, will any reason-
le person assert, that a medical practitioner
nless the structure of his body be supposed
fferent from that of other people) can visit a
tient ten or twelve times, feel his pulse, and
nverse with him, without receiving the in-
ction, if the disease of the patient be conta-
ous, and the practitioner has not, at the
me, a disease of higher degree? We know,
wever, that Physicians, in perfect health,
ve attended as many patients, ill of diseases,
therto reputed contagious, as they could
sit in the day; and yet have escaped. But it

is as absurd to believe, that a person can b exposed to the influence of any power, capabl of producing plague, dysentery, or feve without being affected, by these disease, as tha a large quantity of spirituous liquors, or st mulants still more diffusible, can be applie to living bodies, without producing their usu effects. If it be at all admitted, that contagio is the cause of these diseases; it must also l admitted, that contagion, may sometimes l applied, without producing its effect,—whic is impossible.

Another fact worthy of notice is, that age persons and children, are both seldomer ar less severely attacked, by epidemic and pestile tial diseases, than the young and middle age and women seldomer and less severely tha men. Now, if contagion were the source these diseases, the case would be exact reversed. Old people, women, and childre being more in the way of contagion, would more frequently and more severely attack But the young and middle aged, being mo exposed to the vicissitudes of the atmosphe the principal source, as I shall afterwar endeavour to shew, of those diseases, th aged persons and children, and men more than women, they are consequently more frequently and more severely attacked. It has been a puzzling question to solve, "why old people "and children have been less obnoxious to "plague, dysentery, and fevers, than the young "and middle aged; and women less than "men?" But the solution will no longer be difficult, if it should be proved that these diseases never arise from contagion, but are always produced by certain states or certain vicissitudes of the atmosphere, together with the application of other powers, co-operating in the production of indirect debility. For, it is evident that, to the influence of these states, or vicissitudes, and of these powers, the young and middle aged are always more exposed than old people and children; and men more than women.

Let a person, in the height of a pestilential disease, be removed from the atmosphere which occasioned it, into one more pure, he will communicate the infection to no one. "It has been 'remarked," says Dr. Rush, speaking of the yellow fever of Philadelphia "that this fever 'did not spread in the country, when carried "there by persons who were infected, and "afterwards died with it." In another place he observes, "during four times that it oc-"curred in Charleston, in no one instance, "according to Dr. Lining, was it propagated "in any other part of the state." * Convincing proofs these, that the disease did not depend upon contagion, but upon the state of the atmosphere at Philadelphia, in the one case, and at Charleston, in the other. The various ways, in which the College of Physicians of Philadelphia and Dr. Rush attempted to account for the origin of the contagion, which they supposed had produced the yellow fever o 1793, shews into what inconsistencies the mos sensible men may be betrayed, when they attempt to reason upon false data. Having all taken the *existence* of contagion for granted they only differed with respect to the *origin* o it. The College was of opinion that it wa *imported;* Dr. Rush affirmed that it wa *generated* in the city. A better description cannot be given of the several hypotheses, whic distracted the faculty, upon this occasion, than in his own words, "public report had derived "it" (the contagion) "from several different "Islands; had chased it from ship to ship, and "from shore to shore; and finally conveyed it, "at different times, into the city, alternately "by dead and living bodies; and from these "tales, all of which, when investigated, were "proved to be without foundation, the college "of physicians composed their letter*. It "would seem, from this conduct of the college, "as if medical superstition had changed its "names, and that in accounting for the origin "of pestilential fevers, celestial, planetary, and "demoniacal influence, had only yielded to the "term—*importation*†." But it does not appear that Dr. Rush, in his attempts to trace the origin of the contagion, was more successful than the college. He supposed it to arise from putrid, vegetable exhalation, produced by a heap of damaged coffee, lying on a wharf. But the progress of the disease was not traced, with any certainty, to that focus. It is evident, indeed, from Dr. Rush's own account, that the inhabitants of other streets had been as early and as generally affected, as those of the streets in the immediate neighbourhood of the coffee.

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* Vide an account of the bilious, remitting, yellow fever of Philadelphia, by B. Rush, M. D. page 157.

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* Containing their opinion respecting the origin and treatment of the Yellow Fever. Vide Rush, page 21.

† Vide an Account of the Yellow Fever, page 164.

In this, and in every other case of epidemic and pestilential disease, the existence of contagion would seem to have been uniformly taken for granted, not only without examination, but even contrary to the evidence of numerous facts—a conduct certainly not less unphilosophical in medicine, than in any other department of science.

3—Certain states, or certain vicissitudes of the atmosphere, together with the application of other powers, producing indirect debility, are the cause of all epidemic and pestilential diseases, which affect the same person more than once during life, and have hitherto been reputed contagious.

Every country has its unhealthy season corresponding with some particular period of the year, at which the diseases, peculiar to that country, are more general, and more severe, than at other times. This is observed to happen, in those months most remarkable fer heat, calm weather, or sudden vicissitudes of the atmosphere; and they are nearly the same in all parts of the world. In Europe, Asia, Africa and America, from July to October, with little variation, includes the most unhealthy portion of the year. In some places indeed, as Aleppo, that happens from April to July; but always with a certain regularity, coinciding with periodical states of the weather. The diseases, which annually arise from this source, are not always general or severe. It is only when the heat, calm weather, or vicissitudes of the atmosphere, have been uncommon, hat the ordinary diseases of the season arise to degree, which constitutes epidemic and pesti-ential diseases.

From every record of epidemic and pesti-ential diseases, it would appear, that they ave their stated periods of recurrence; that hese periods are such months, as are most remarkable for vicissitudes in the temperature of he atmosphere; that they become general, only in years in which these vicissitudes are extreme; that they do not occur in seasons, when the degrees of heat or of cold, however ntense, are equable; nor in years when the

state of the atmosphere remains tempere
throughout: and that they uniformly cease
with the establishment of an equable state o
the atmosphere, whether the weather be hot o
cold.

The yellow fever in America "appeared si
"different times about the 1st or middle o
"August, and declined or ceased about th
"middle of October—viz. in 1732, 1739, 174
"and 1748 in Charleston; in 1791 in Ne
"York; and 1793 in Philadelphia."* In 179
the yellow fever appeared also in different par
of the West Indies†. Attempts were made, i
the Islands, to trace the contagion to the Co
tinent. On the Continent it was traced bac
to the Islands. But why should we hesitate
believe, that the same general causes whi
produced unusual vicissitudes of the atmo
phere, in the one country, should extend the
influence to the other? In the same year, a
the same season, the English settlers, on t
coast of Africa, were seized with a fev
which proved fatal to a great number of the

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* Vide Rush on the Yellow Fever.

† Vide Chisholm on the Malignant, Pestilential Fever,

It happened, at this period, that a ship arrived from Boullam, on the coast of Africa, at Grenada, in the West Indies. And hence the contagion was supposed, by Dr. Chisholm and others, to have been imported in that ship. Was it necessary, it might easily be shewn, that these suppositions were adopted upon very slight grounds. But if the existence of contagion can be disproved upon general grounds, it would be superfluous to investigate every particular circumstance in its favour, that may have been hastily assumed as a fact.

In Aleppo, according to Dr. Russel, the European inhabitants regularly shut themselves up, in their houses, every year, at some period between April and July. And the rich natives begin to adopt the same plan, as far as their customs will permit them to do, without scandal. Erom this fact, it appears that the plague occurs at Aleppo, in a state less or more mild, lmost annually, and *that it commences and eases at certain known periods.* But it has been emarked there that, in its most severe state, his disease recurs only at periods of ten years, r there-abouts—a regularity, which cannot, pon any known principle, be attributed to a power of such casual application, as contagious matter.

It has farther been observed of the plague, "the winter puts an end to it at Constantinople; "the summer destroys it in Egypt." In fact, what epidemic or pestilential disease has been known to occur, with severity, at these periods of the year? But, in order to account for this, will it be said that contagion is destroyed, both by heat and by cold? The assertion would certainly be absurd. Besides the fact can be much better explained. At these periods, the body is not so liable to disease, because it is not exposed to the effects of heat and cold dryness and moisture, tempestuous and calm weather, suddenly and frequently alternated These vicissitudes are most remarkable in sprin and autumn, which accordingly are the seasons most fertile of diseases, in all parts of the globe

It is a curious circumstance, and much t our present purpose, that the belief of the Turks in the contagious nature of the plague, ha considerably increased, since their commu nication with Europeans has become more ex tended. Formerly there was no want of ser vants, or relations, to undertake every necessar office about the sick, the same as in any common distemper; but now, it is difficult to procure even mercenary attendants. “I have met,” says Dr. Russell, speaking of the plague at Aleppo in 1760, “with several instances, even “in Turkish houses, where the mistress of the “family was not only ill attended, but even “abandoned through the timidity of her daugh“ters and slaves. I apprehend the dread of “contagion gains ground among the Mahom“medans, in all parts of Syria, where the Eu“ropeans have much commerce.” Mahommed, having probably perceived the bad consequences that would result from such a dread, condemned the belief of diseases being spread by contagion, as *impious*. And this at least shews, that he plague has not always been supposed, by he Turks, to arise from contagion; or if it has, that the belief was deemed injurious. With the example of European credulity before them, he modern followers of Mahommed may, in no long time, put more faith in contagion, than n this law of their prophet. But in such a enunciation of faith, even a Christian will ave little cause to rejoice. If it should appear o be only a substitution of one error for another, Europeans will not have much reason to ridicule the former stupidity of the Turks; nor to boast of their own superior penetration, in introducing among them a belief in the contagious nature of pestilential diseases.

Physicians, having observed the dependence of epidemic diseases upon the state of the atmosphere; their uniform appearance under some states, and cessation under others; could not well reconcile these facts with the hypothesis of contagion. But a reconciliation was, at all events, determined upon. The state o the atmosphere was made to act upon the matter of contagion, in such a manner as to explain every phoenomenon. Is the weather hot when an epidemic commences, heat gives activity to contagion; is it cold, cold is favourabl to contagion; is it dry, the contagion is concentrated; is it wet, diluted: even vicissitude set it in motion. But should the epidemic happen to cease, during any of these states of th atmosphere, this may with equal facility b accounted for, by assigning to the *same* powers as has frequently been done in medical reasoning, *different*, or even *opposite* modes of operation. Let the existence of contagion be onc admitted as a fact, and there is nothing mor easy than to trace its origin to some ideal source. The most obvious, and therefore the most frequently insisted upon, is contact with some person, ill of the same disease. But as the person, who happens to be first seized, could not have received the infection in that manner, it was found necessary to refer it to other sources. Even with those advantages, however, it was often difficult, and exercised the ingenuity of the learned, to discover the origin of particular epidemics. The imputed sources of those calamities have become at length so numerous, that it now requires little labour to trace the origin of all diseases to some one, or other of them. If, for instance, it cannot be traced to actual contact, it will probably be discovered, that the patient has, at some recent period, been exposed to the effluvia of rotten hemp, flax, coffee, cabbage, onions, black pepper, or potatoes; for all of these powers have been said to produce epidemics. But in years, when these diseases are so highly pestilential, that the effluvia, arising from a heap of rotten vegetables, might seem too trifling a power to produce such important effects, recourse may still be had to the importation of contagious matter, in bales of goods from the Mediterranean; or, with the ingenious Gibbon, to the generation of it, by swarms of putrid locusts, in Egypt. These hypotheses, were they not supported by the autority of celebrated names, are almost too ridiculous for refutation. That a parcel of rotten vegetables should produce a disease, that is contagious, and capable of producing desolation and death, over a populous city, ought not certainly to be credited without proof; and with respect to proof, it does not appear that there is any, excepting that, during the prevalence of epidemics, vegetables have become putrid. Were putrid vegetable exhalation ever the cause of a contagious disease, it would spread in an evident and regular progression, affecting first those who are nearest to its source. There could be no possibility of mistaking or overlooking the cause. But as no such progress has ever been ascertained, and as it might have easily been traced, had there been any truth in the opinion, it is every way inconsistent with just reasoning to admit, that putrid vegetable exhalation can be the cause of contagion.—I mean not to deny, that putrid vegetable exhalation may produce disease; but the disease will not be contagious. There cannot be a doubt that putrid vegetable exhalation is a power, capable of producing disease, in its immediate neighbourhood; but it is equally certain, that it never can occasion an epidemic or pestilential disease, over a whole country, or city. The putridity of vegetables, and the epidemic diseases of animals, are probably occasioned by the same power, viz. a certain state or certain vicissitudes of the atmosphere. That kind of weather, or that disposition of the surrounding elements, which occasions an uncommon mortality, among animals and vegetables, will also produce an uncommon degree of putrefaction, among these substances, in their dead state.

Could the history of all epidemic and pestilential diseases of animals be minutely traced, I am well convinced it would be found, that they have uniformly been attended with correspondent diseases of vegetables, in that particular part of a country, to which they have been confined. For, as all living bodies are subject to the same laws,* it is evident that any power, which can produce general disease in animals, will have the same effect upon that portion of vegetable substances, to which it is applied; and *vice versa*. Accordingly, those diseases of indirect debility of vegetables, known to farmers by the terms *rust* and *blast*, have often been observed to occur, at the same time with epidemic diseases among animals. And the reason why such a coincidence has not always been expressly noticed, is probably, that the subject has not been considered in this point of view. If upon investigation, such a coincidence, then, should be found invariably true, will it be said that contagion may be communicated from animals to vegetables, and from vegetables to animals?

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* Vide prop. I. View of the Science of Life.

When particular districts of a country, whole nations, or considerable portions of a continent, are suffering from a scarcity of grain, will it be said that the disease of vegetables, which is the cause of the scarcity, was produced, not by the state of the atmosphere, but by contagion? In this case, how is the contagious matter to be traced? Is it wafted, as it were, by a magic influence, from field to field, — over mountains, rivers, lakes, and oceans? The infectious distance would, in that case, be wide indeed! But I apprehend it will scarcely be contended, that the epidemic diseases of vegetables are contagious. And in regard to animals, the opinion does not appear at all more probable, excepting from the single circumstance of their not being rooted to the soil. Would it not be more rational to admit, that the diseases, in both cases, are produced by the operation of some such general power as the states or the vicissitudes of the atmosphere, to the influence of which animals and vegetables are equally exposed?

Of the numerous facts, by which this proposition is supported, it will suffice to quote a few. As Dr. Rush's account of the yellow fever of Philadelphia is, perhaps, the best history, that has been given, of any epidemic, it may often, with propriety, be referred to. “There ‘was something in the heat and drought of the ‘summer months,” (1793) “which was uncommon, in their influence upon the human body. Labourers every where gave out, (to use the common phrase) in harvest, and frequently too when the mercury in Farenheit's Thermometer was under 84*****. The crops of grain and grass were impaired by the droughts.”

It appears, from several observations, that here was, that year, an uncommon calmness f the weather.

"In the year 1762, the bilious yellow fever "prevailed in Philadelphia, after a very hot summer, and spread like a plague, carrying off "daily, for some time, upwards of twenty persons." Can it be doubted that these states of the weather will produce disease, both among animals and vegetables? And, if the operation of such an obvious power be adequate to explain the phoenomena of pestilential diseases, what need is there of adopting an ideal one, like contagious matter, to account for them?

Mr. Potter, in a letter to Dr. Rush, dated from Caroline county, Maryland, 1st November 1793, says, "it is an invariable maxim "here, both among physicians and farmers, that "if the wheat be damaged by rust or blast, a "contagious dysentery is soon to follow."*

Previous to the occurrence of every epidemic, something unusual, in the state of the atmosphere, has always been remarked. A yellow fever appeared at Cadiz, after a hot and dry summer, in 1764; and at Pensacola, in similar circumstances, in 1765. Was the contagion traced, in this case, from Cadiz to Pensacola, by a direct or circuitous channel, or was it traced at all?—That the yellow fever of Philadelphia, in 1793, depended upon the states or vicissitudes of the atmosphere, evidently appears from the following observations, communicated to Dr. Rush, by a gentleman, who resided occasionally in southern and tropical countries. He informed him, "that he had observed, in the month of "July, several weeks before the yellow fever "became general, a peculiar and universal "sallowness of complexion, in the faces of the "citizens of Philadelphia, such as he had ob-"served to precede the prevalence of malignant "bilious fevers, in hot climates." Dr. Dick had observed "the same appearance in the faces of "people in Alexandria, accompanied in some "cases, by a yellowness in the eyes, during the "last summer" (1793) "and some time before "violent, bilious, fevers became epidemic, upon "the banks of the Potowmac."* A change so gradual and general in the appearance, both of animals and vegetables, can never be explained by admitting contagion, but is easily and satisfactorily accounted for, by supposing the states or vicissitudes of the atmosphere to have been the noxious power. "It appears "farther, from the register of the weather, that "there was no rain between the 25th of August, "and the 15th of October, except a few drops, "hardly enough to lay the dust of the streets, "on the 9th of September, and the 12th of "October. In consequence of this drought, "the springs and wells failed in many parts of "the country. The dust, in some places, ex-"tended two feet above the surface of the "ground. The pastures were deficient or "burnt up. There was a scarcity of autumnal "fruits in the neighbourhood of the city. But "while vegetation drooped or died, from the "want of moisture in some places, it revived "with preternatural vigor, from unusual heat, "in others. Cherry trees blossomed; and apple, "pear, and plumb trees bore young fruit, in "several gardens in Trenton, thirty miles from "Philadelphia, in the month of October.

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* Vide Rush on the yellow fever, page 181. If the word "*contagious*" had been omitted, the observation would have been more correct.

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* Vide Rush, page 183.

"However unoffensive uniform heat, when "agitated by gentle breezes, may be; there is, "I believe, no record of a dry, warm, and "stagnating air, having existed for any lenght "of time, without producing diseases. Hip-"pocrates, in describing a pestilential fever, "says, the year in which it prevailed, was with-

"out a breeze of wind. The same state of "the atmosphere, for six weeks, is mentioned "in many of the histories of the plague, which "prevailed in London, in 1665."*

Thus all the facts stated by Dr. Rush, and many of his observations tend to prove, that the yellow fever of Philadelphia, in common with other epidemics, was produced by the states or vicissitudes of the atmosphere, and not by contagious matter, imported, or generated in the city.

Pestilential diseases are neither so frequent nor so fatal in modern, as they were in ancient times. Cities are now more commodiously built; the mode of living is improved; and every circumstance that can contribute to the preservation of health better understood. Is it not from these changes, in the state of society, that London, Paris, Madrid, Lisbon, and Marseilles, are now much less subject to epidemic diseases than formerly? And in the progress of improvement, may not these diseases entirely disappear? The inhabitants of Grand Cairo, according to Mr. Savary, are heaped together by thousands. Two hundred citizens there occupy less space than thirty at Paris. Thirty citizens of Paris occupy less space than ten citizens of London. Twenty citizens of Grand Cairo, therefore, occupy less space than one citizen of London. The manner in which the citizens of Grand Cairo are thus crouded together, would alone seem sufficient, in a stagnant state of the atmosphere, to produce pestilential diseases of the highest degree.

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* See Rush pages 109—110.

The large commercial cities, which have been most frequently ravaged by the plague, are, for the convenience of sea-ports, built in low and unhealthy situations. Their streets have generally been irregular, crouded, and dirty. In these cities, therefore, pestilential diseases always commence. This circumstance together with that effect of self-love, which prevents us from discovering the origin of any evil with ourselves, probably gave rise to the idea that contagion was imported in bales of goods or even in parcels of old clothes, from distan countries. The epidemic of a season, appearin generally in several places at a time, b enabling the inhabitants of one place to trac it to another, has also served to strengthen th same opinion. But may not similar states the atmosphere occur, in the same season, i

Egypt and in Syria, in Damascus and in Aleppo, in Grand Cairo and in Marseilles, in Smyrna and in London, in the West India Islands and in America? And will not these similar states produce pestilential diseases of a similar appearance? The plague, indeed, will never appear with similar symptoms in London and in Constantinople, because the states or vicissitudes of the atmosphere, in these two places, can never be exactly alike. But if it depended upon a power, like specific contagion, which must be the same in all places, the symptoms would every where appear with an uniformity similar o those of small-pox.

Contagion then, it would seem, cannot explain the phoenomena of pestilential diseases, vithout the assistance of the states or vicissitudes of the atmosphere; but the states or vicissitudes of the atmosphere will explain them, without he assistance of contagion. Here I will again vail myself of the authority of Dr. Rush, as far s importation is concerned. "The report of the College of Physicians has served to confirm me in an opinion, that the plagues which desolated most of the countries in Europe in former centuries, and which were always said to be of foreign extraction, were "in most instances of domestic origin. Between the years 1006 and 1680, the plague "was epidemic 52 times all over Europe. It "prevailed 14 times in the 14th century. The "state of Europe in this long period is well "known. Idleness, a deficiency of vegetable "aliment, a camp life from the frequency of "wars, famine, an uncultivated and marshy "soil, small cabins, and the want of cleanliness "in dress, diet, and furniture, all concurred to "generate pestilential diseases. The plagues "which prevailed in London every year, from "1593 to 1611, and from 1636 to 1649, I "suspect were generated in that city. The "diminution of plagues in Europe, more "especially in London, appears to have been "produced by the great change in the diet and "manners of the people; also by the more commodious and airy forms of the houses of the "poor, among whom the plague *always* make "its first appearance. It is true these plague "were said by authors to have been imported "either directly or indirectly from the Levant "but the proofs of such importation were in "most cases as vague and deficient, as the "were of the West India origin of our lat "epidemic. The pestilential fevers, which hav "been mentioned, have been described by authors, by the generic name of the plague."*

Farther, there cannot be a doubt that the eterioration and scarcity of grain, which take lace in bad seasons, are capable of giving aditional force to the effect of the other powers, hich produce epidemic and pestilential disases among animals; and that years of peslence are also years of famine. Famine has ot, more than pestilence, been so frequent in odern as in ancient times, and for similar asons. Grain is now more easily transported, om one country to another, than formerly; which means the effects of a local scarcity e more easily obviated. Upon this subject, I ve met with some observations very much to e purpose, by an elegant and ingenious French riter, the Marquis de Chatellur. The book t being at hand, I regret that I cannot now ote his words. But the passage alluded to ill be found in a note to page 195 of the first lume of his profound, erudite, and sublime say on public happiness:—a book of rare erit, and very little known.

Why do pestilential diseases *always* make eir first appearance among the poor? Has

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Vide Rush on the yellow fever, page 165—166.

contagious matter an instinctive attachment to this class of men? No. But they are constantly more exposed than the rich, to the principa powers, which produce pestilential diseases, viz certain states or certain vicissitudes of the weather, hard labour, low diet, and other privations

The vicissitudes of the atmosphere constitut a power, great, evident, and extensive, in its effects upon the animal and vegetable world:— a source, to which the epidemic and pestilentia diseases of living bodies may, with certainty be traced. Whereas contagious matter is power that has uniformly been taken fo granted, without examination; of which th existence, in epidemic and pestilential diseases is even disproved by a numerous induction o facts; and, if admitted, is incapable of explaining their phoenomena.

From all these considerations, I conclude that no general disease, excepting such as occu only once during life, is contagious. And tha all epidemic and pestilential diseases, whic occur more than once during life, and hav hitherto been reputed contagious, depend upo certain states, or certain vicissitudes of th atmosphere, together with the application o other powers, producing indirect debility.

Viewing this as not merely a question of idle medical disputation, but as one of the utmost practical importance, I regret that neither my abilities, nor my situation, enable me to do it that justice, which it certainly deserves. The attempt, which I have made, may, however, be the means of calling forth the observations of others, better qualified to illustrate the subject. Whether the existence of contagion, in epidemic and pestilential diseases, be ultimately proved, or disproved, a discussion, and decision of the question must be attended with considerable utility. Let us take a view of the pernicious consequences, which result from the opinion now received, supposing it to be false; and contrast them with the benefits that would arise from a contrary one, supposing it to be true.

The consternation and mortality, occasioned by epidemic diseases, must always be greatly increased, by a belief in their contagious nature. Those who are yet well, will be the more readily affected; and those who are ill, will be in greater danger of suffering, from the desertion of timid relations, or of mercenary attendants. What serious evils may not the dread of contagion produce, among the uninformed multitude, when it can occasion such scenes as the following, among sensible men of the medical profession? In 1665, we find Dr. Hodges prescribing, from his parlor window, for patients in the streets of London; and at a later period, Dr. P. Russel prescribing from a chamber window, fifteen feet above the level of the streets at Aleppo. Dr. A. Russell's candid account of the manner, in which he prescribed, is worthy of note; both as it tends to disprove contagion, and to shew the pernicious consequences of believing in it. "In the two preceeding years" (he wrote in 1744) "I had prescribed for the "sick, chiefly from the accounts brought me "by a person, whom I employed to visit them; "for though before shutting up, I was often, "in spight of all my precautions, deceived by "false representations of the case, and led to "visit some of the infected; yet I avoided it to "the utmost of my power: but this year the dread "of contagion (like that of other dangers to "which one has been long exposed) being much "worn off, I attended the sick in the plague, in "the same manner as those labouring under "ordinary fevers." Could Dr. Russel, or his deputy, have attended the sick, with impunity, if the disease had been contagious? In other words, can a power be applied, without producing its correspondent effect? I know not by what refinement of sophistry, the force of this objection can be eluded.—To come down to a period still more recent, some of the physicians of Philadelphia are said to have fled the city, during the prevalence of their late epidemic; a conduct that must have added both to the consternation and mortality of their patients. The effects of a popular belief in such opinions are, in my estimation, no less injurious to mankind, than they are humiliating to the medical profession. What would be said of a military officer, who deserted his post at the sight of an nemy, leaving his fellow soldiers to fight the battle? During the rage of an epidemic, phyicians may be looked upon as general officers, n whom it is always regarded more shameful o abandon the field of battle, than in private oldiers.

If, on the other hand, a belief in contagion was entirely laid aside, the European inhabitants of Aleppo, and other places subject to the plague, would no longer shut themselves up in their houses, for fear of contagion. They would only remain at home occasionally, to avoid the influence of the sun, or vicissitudes of the weather. Instead of a constant confinement for several months, they would only think it necessary to refrain from going abroad during the hotest part of the day; or to take precautions against the morning and evening fogs. Thus the dread inspired by the apprehensions of infection, would happily be banished from their minds; and that alone would be a powerful mean of protecting them from disease. It is not supposed, however, that the custom of shutting up is useless. The utility of it is evident; and it is as evidently founded upon a principle, very different from that of avoiding contagion. By confinement, the inhabitants of Aleppo avoid exposure to heat, and the vicissitudes of the weather, which are the real source of the plague. But their confinement, if regulated upon principle, need neither be so constant nor so anxious.

Another advantage that would result from rejecting the doctrine of contagion, in pestilential diseases, is that the quarantines usually exacted of ships, coming from places suspected of contagion, would no longer be considered necessary. The hardship, or rather the cruelty of such ordeals, is too evident to require a comthe manner supposed, the injury to individuals must of course be suffered, on account of the community. But if it be proved that this cannot happen, the restriction must appear exceedingly absurd. Is it probable, that London being exempted from pestilential diseases, for many years past, will be imputed to the wonderful strictness, with which Mediterranean ships have been made to perform this forty days farce?

Above all, the adoption of this theory, by recalling physicians from a wrong tract of investigation, would probably be the means of enabling them to apply principles to the cure of all epidemic diseases, hitherto so often fatal, which would render them little more dangerous, than common fevers are at this day.—Instead of wasting time in tracing contagious matter from city to city, they would endeavour to discover what are the particular states or vicissitudes of the atmosphere, which produce epidemic diseases; what are the causes of these vicissitudes; and what are the best modes of counteracting their effects upon the human body.

This subject is highly worthy of investigation. For although it may be said that, as the stimulant powers, which are found to cure epidemic diseases, afford a proof that they depend upon a very great degree of indirect debility, and that therefore a minute acquaintance with the powers which occasioned them is not necessary to guide the practice; yet it must also be admitted, that every link, in the chain of knowledge, is a valuable acquisition.—There is not a fact in nature, from which some useful inference may not be drawn.

OBSERVATIONS ON THE CURE.

Whatever be the powers that produce epidemic diseases, it is evident, from those which are found to cure them, that they all depend upon a high state of indirect debility. Fevers and dysentery have of late, every where, yielded to the powers of mercury, and other stimulants of the most diffusible kind. That is, those medicines have been found more successful, than any that were ever used before. If eight grains of calomel, and four grains of opium, repeated every two, three, or four hours, will cure a fever, or a dysentery of a certain degree, will not the same medicines produce the same effect in plague, if given in quantities proportionate to the force of the disease? Ought not the physicians of Aleppo to give a fair trial to a medicine in plague, which has been found so successful in other epidemics? As plague, however, is a disease, by all accounts, of a very high degree of exhaustion, it may sometimes be necessary to go the length of, from fifteen to twenty grains of calomel, or even more, in repeated doses. The duration of the action of each dose, should regulate their repetition; but that does not seem to be yet accurately ascertained. It appears, as far as I have been able to observe, that the intervals, between the doses of mercury, ought not to be longer than two hours perhaps not longer than one.—In exhibiting this medicine, it is a fact worthy of remark, and deserves to be particularly remembered when large doses are required, that by a sudden subduction of it, the patient is apt to have a very sore mouth, a violent salivation, and sometimes an alarming discharge of blood from the fauces. When any of these symptoms unexpectedly occur, it will be found, that the patient has suddenly left off his medicine, or has taken it in such an irregular manner, as to produce similar effects. This will often happen, from the imprudence of patients, in the hands of the most skillful physicians; but it perhaps more frequently occurs, from an entire ignorance of the fact. This fact, which is both new and important, is considered at greater length, in my "treatise on the action of Mercury, upon living bodies etc." of which a new and improved edition is now in the press. But it cannot be too often inculcated, particularly when it is proposed to give this medicine in a disease, that will probably require its exhibition in unprecedented quantities. Suppose a case of plague to require the exhibition of a scruple of calomel every two or three hours, if it was suddenly left off, an alarming hemorrhagy would in most cases ensue. It would be of considerable advantage to the practitioner, to know that this effect was produced by the too sudden subduction of the high stimulant power, which had been for some time previously applied to the body; and that it may be prevented by the regular exhibition and gradual reduction; or removed by the re-application of the same power, or the substitution of others equivalent in force. Vicissitudes in the application of substances, used in medicine, will produce disease, as well as vicissitudes in the state of the atmoshere, or in the force of any other exciting ower. But an application of the same powers, a due degree, will remove the diseases, which excessive, or a deficient application, or alterations in the force of them may have occasioned.

The terms excess and deficiency, in the aplication of external powers to living bodies, o not relate to the sum of stimulus usually pplied in a state of health, but to the state of he excitability at the time.

TIGHT
GUTTER